Response to Reviewer 2

We are grateful to the reviewer for the constructive and thorough review. It is clear that the manuscript would benefit from an improved and more clear formulation of our modelling approach. We will fully clarify these issues in the revised version of the manuscript. At the same time, we would like to make it clear that we respectfully disagree with the reviewer's main criticism. We welcome the chance to defend our approach in an open scientific discussion, and we truly hope for the understanding and openness of the reviewer to our explanations.

Reviewer's comments are in indented blocks and in italic fonts.

Based on the reviewer's comments, we have got the impression that our explanation of the physical reasoning for our discharge parameterization given in the middle of Section 3 was not visible enough. We wrote: "The idea here is to largely capture two aspects of fast ice flow with this parameterization: the fast outlet glaciers themselves, as well as the rather fast flowing ice in the catchment regions upstream of the outlet glaciers. The ice thickness h describes the amount of ice that can be brought to the outlet glaciers. And the power of the inverse distance to the coast $(1/l)^3$ can be regarded as a statistical measure of the outlet glacier density: if the ice margin is far away from the coast, it is very unlikely that any outlet glacier has contact with the ocean and there is only minor calving flux into the ocean, while one would expect a large calving flux for small distances to the coast." We will move these sentences to the beginning of section 3 in order to explain our idea before giving formulas.

The reviewer admits that the ice discharge into the ocean occurs through narrow ice streams into fiords, which cannot be resolved in the modern GrIS (Greenland Ice Sheet) models with typical resolution of 10 km. The reviewer also agreed that "*a parameterization of ice discharge would indeed be very useful for ice-sheet models of this kind*". However, the reviewer believes that "*the parameterization developed here is not appropriate and intrinsically highly problematic*". The reviewer's dislike for our approach is based on the false premise that our parameterization implies "*double counting*" of solid ice discharge into the ocean and violates mass conservation. In reality there is no "double counting" and our model is mass conservative, which directly follows from Eq. (5), which conserves the mass by its construction; note that the right hand terms of the equation sum up the different contributions to (temporal change of) mass. The mass conservation aspects can be understood better if Eq. (5) (there is a sign error in the equation; it must read $-d$) is rewritten for its total amounts (simply integrated over ice area). This reads

$$\frac{dV}{dt} = B - B_M - D_{\mathrm{expl}} - D_{\mathrm{par}}, \quad \text{(A1)}$$

with the ice volume V, the time t, the total surface mass balance B and the total basal melt B_M. The explicit ice discharge D_{expl} is the total ice discharge simulated by SICOPOLIS via the large-scale ice flux and the parameterized total small-scale ice discharge is denoted D_{par} here. The model together with the observational constraints freely determine, which of the two kinds of these ice discharges prevail in the simulations. Further on, we confirmed the mass conservation by a detailed analysis of the simulated mass balance components of the GrIS. Of course, the reviewer is perfectly right that in reality the ice should be in direct contact with the ocean to calve, but for the modern GrIS, most of such contact occurs in narrow fiords, which cannot be resolved in the model. When this "reality" is projected onto the 20 km grid, only 15% of marginal ice sheets grid cells are in direct contact with the ocean and still total solid ice discharge from the GrIS exceeds 50% of its surface mass balance (SMB). This is a well-known problem and there are several ways to deal with it:

1) Completely disregard this problem. In this case to be in contact with the ocean, the simulated ice sheet spreads over most of Greenland and both the area and the volume of the simulated GrIS are overestimated by ca. 20% (e.g. fig 9b). Such an ice sheet represents a very unfavourable initial condition for future climate simulations, because in this case for several centuries most of the GrIS mass loss will originate from areas, where ice does not exist in reality.

2) Compensate this problem by enhanced surface melt. This approach is also not satisfactory because, as it is shown in our paper, it strongly overestimates SMB sensitivity to temperature change and makes GrIS more unstable than it is in reality.

3) To make model spin-up using prescribed present-day GrIS elevation and extent and to use anomaly approach for future projections. This approach allows one to overcome the problems of the first two approaches, but the use of the prescribed modern GrIS topography prevents model validation for present-day, as well as for simulations of past climates when the GrIS was very different form present.

Our approach is based on decomposing (not double counting!) the solid ice discharge into two components: Namely, (i) the large-scale, i.e. ice flow into the ocean explicitly resolved by the coarse-resolution ice sheet model (the reviewer named it "*implicit*", but we prefer to name it "explicit") and (ii) parameterized (sub-grid) ice discharge through the fast moving narrow outlet glaciers. Note that under present-day conditions, most ice discharge in our model occurs through the second process, which agrees with observations (Rignot and Mouginot, 2012). In spite of its simplicity, our approach is free from the problems of the above mentioned approaches. It allows us to simulate a rather realistic present-day GrIS geometry without overestimation of surface melt. In addition, this approach is suitable for modelling of climates very different from the present one.

> *In fact, ice discharge is already implicitly included in SICOPOLIS anyway – as all ice that reaches the ocean is removed passively. The calving flux could easily be calculated from the ice flux at ocean-bordering gridpoints*

Of course, in the case, when the modelled ice sheet comes into direct contact with the ocean, the explicit ice flux into the ocean is computed by SICOPOLIS, but under present-day conditions over the most ice sheet margin, such contact is absent (on the model grid). This is the reason for including the parameterization of sub-grid calving flux. This parameterization may look unusual in the framework of ice sheet modelling, but in fluid dynamics a separation of the flow into explicitly resolved and sub-grid (eddies, turbulence, etc.) parts is common practice.

> *If the scheme of the model conserves mass well, this flux should add up with SMB to give a zero total mass balance for a steady state*

As we already stated above, the model conserves mass. As a result, in equilibrium the sum of all components of the GrIS mass balance (accumulation, surface and basal melt, explicit and parameterized discharge) is equal to zero with a good accuracy.

> *The parameterization proposed here in fact does something else. It is effectively an additional surface mass balance term that has no direct relevance to the process of calving.*

This is incorrect. Although the term d does have the same unit as the surface mass balance (m/yr), it has a completely different nature – it represents the lateral divergence of the ice flux through fast processes. Unlike the surface melt term m in the surface mass balance, the term d does not explicitly depend on climate conditions and it depends on ice sheet elevation in opposite way to the term m. Indeed, m becomes larger with thinning of the ice sheet while d becomes smaller. As a result, introducing the term d does not explicitly affect sensitivity of GrIS to temperature, contrary to the attempt to get the right shape of the GrIS through enhanced surface melt, as done by other workers.

> *In case parts of the ice sheet still border the ocean, the total mass balance is then negative in their model (because the passive calving flux needs to be subtracted)*

This is not true – in steady state ($dV/dt = 0$ in Eq. (A1), this document) the total mass balance is always zero (see above) irrespectively whether the (modelled) ice sheet is in contact with the ocean or not:

$$B - B_M = D_{\mathrm{expl}} + D_{\mathrm{par}}.$$

> *In case the ice sheet has retreated on land, there cannot be any ice discharge in reality*

In “model reality” it can. In the coarse model resolution, the real GrIS is not in contact with the ocean over most of its margin, even though solid ice discharge exceeds 50% of SMB. The only way to get the modelled ice in contact with the ocean (without overestimation its volume and area) is to use spatial resolution finer than one km, which is impractical for long-term simulation. If the reviewer knows a more elegant way to circumvent this problem, we would be very interested to learn about it.

> *That seems intrinsically wrong, and explains why the parameterization does not perform well for the southwestern part of the ice sheet that is already nowadays largely land-based.*

We do not claim that our approach is perfect, but it improves the model's performance compare to the standard version without such parameterization. We do not see any relationship between model deficiencies and the "land-based" ice sheet, since at present almost the entire GrIS is "land-based".

> *When applying the model to the Eemian or future warmer climates, this artefact of the method will only become stronger.*

Under Eemian or future warming conditions, the ice sheet retreats inland, the distance between the ice sheet margin and the ocean increases and, as shown in our Fig. 3b, the parameterized solid ice discharge decreases rapidly but does not cease completely, which we believe is absolutely realistic. The recent paper by Morlighem et al. (2014) demonstrates that deeply incised submarine glacial valleys spread far into the GrIS. Therefore, solid ice discharge can continue even when the margins of the ice sheet retreat far from the ocean, while outlet glaciers from the ice sheet still can have contact with the ocean via fjords. This is not resolved by coarse resolution models, but captured by our parameterization. Therefore, we do not agree with the reviewer that the "artefact becomes stronger" for warmer climates.

> *In summary, the model seems to violate mass conservation, there is an issue of double counting and the process of ice discharge is disconnected of what happens at the ice-ocean boundary.*

We have shown above that this statement is incorrect.

> *In my eyes a more credible parameterization of ice discharge should focus on thinning at the contact point between ice sheet and ocean. How this affects inland ice dynamics is part of the ice-dynamic model.*

As we discussed above, this is not possible to achieve with the current resolution of GrIS models.

> *Instead of focusing on the amount of ice discharge (which is in fact disguised as a surface melting term) and its fraction of the precipitation (MBP), it would help to show all mass-balance components and their evolution though time.*

We will follow this reviewer recommendation and will show all components of the mass balance in the revised version of the manuscript.

> *Incidentally, do the authors mean 'accumulation' when they refer to 'precipitation' or do they not account for the fraction of rainfall?*

Under "precipitation", we mean precipitation (solid plus liquid) and not accumulation. This was done to simplify the comparison with the regional climate models since refreezing of meltwater and refreezing of precipitation can be treated in different ways in different models. Total precipitation is somewhat higher than accumulation.

> *I am surprised that the authors choose to vary only the parameter cm in the melt parametrization. … If one changes cm one ought to change also 'lambda' and likely 'tau s – transmissivity' to have a reasonable fit to any mass-balance data.*

Due to large uncertainties in the components of the GrIS mass balance, it provides only a limited constraint on values of model parameters c_m and λ (τ was taken from direct observational data). In principle, one can vary both of them. However, even for such a computationally efficient model as REMBO-SICOPOLIS, the number of perturbed parameters cannot be too large. For consistency with our previous publications, we chose c_m as the free surface mass balance parameter, since it was found to be relevant for the sensitivity of the melt model (Robinson et al. 2011).

> *Thirdly, I miss a graph showing the actual temperature forcing over the ice sheet during the Eemian in particular as this can be compared to the NEEM ice core record.*

The real temperature forcing is the temperature at the lateral borders of Greenland, which is shown in Fig. 1 of Robinson et al. (2011) and is the same as used in this study. The temperature curves for different model versions over the upstream source of NEEM ice, are very similar to that which are shown in Fig. 9c in Robinson et al. (2011) for the GrIS summit. Whether they can be directly compared with the NEEM ice core d18O record is an open question. This record implies 8±4 °C annual mean Eemian warming over the GrIS. Such strong temperature rise is hard to reconcile with modelling results and other palaeoclimate proxies. The relationship between d18O and temperature is still not well understood.

> *To conclude, I believe the paper cannot be published provided the authors are able to convincingly counter the criticism raised above*

We believe that with this response we convincingly countered reviewer's criticism.

Response to minor comments

> *GIS should be called GrIS throughout the manuscript to avoid confusion with the established meaning of GIS: Geographic Information Systems.*

Agreed. We will use the abbreviation GrIS instead of GIS throughout the entire paper.

> *What is the origin of the bedrock data in their version of SICOPOLIS applied to Greenland?*

This is the bedrock by Bamber et al. (2001), as in Robinson et al. (2010).

Additional References

Morlighem M, Rignot E, Mouginot J, Seroussi H, and Larour E, Deeply incised submarine glacial valleys beneath the Greenland ice sheet. Nature Geosci. 7, 418-422, 2014.